

# Organizing Knowledge and Behavior at Yale's Institute of Human Relations

By J. G. Morawski\*

IN 1929 JAMES ANGELL, president of Yale, announced plans for a unique teaching and research center for those fields "directly concerned with the problems of man's individual and group conduct. The purpose is to correlate knowledge and coordinate technique in related fields that greater progress may be made in the understanding of human life. . . . The time has certainly come once again to attempt a fruitful synthesis of knowledge." The *New York Times* described the experiment as dismantling the disciplinary "Great Wall of China" and compared it with the Renaissance transformation of knowledge.<sup>1</sup> The Institute of Human Relations (IHR), as the center was named, received over \$4.5 million from the Rockefeller Foundation for its first decade of operation. The IHR was to be more than a research haven for social scientists, doctors, and lawyers: it was in actuality an experiment, an attempt to construct a cooperative and integrated scientific enterprise.

The Institute's objective—an integrated, synthetic science, cooperatively managed and oriented to eventual practical applications—drew upon new ideals in the human sciences. By the early twentieth century postulates of moral autonomy and rational cognition seemed to be yielding to complex conceptions of action that stressed multiple, interdependent causation. Nineteenth-century idealism and positivism seemed to be giving way to ideas about an antireductionist, antiformalist, and pragmatic science. The limits of rationality and of simple mechanical models of action were indicated by innovations in biology and physics and reiterated in strong fashion by Freud and his associates.<sup>2</sup> To the founders of the Institute these new conceptions suggested the need to transcend disciplinary boundaries and emphasize unification over specialization.

\* Department of Psychology, Wesleyan University, Middletown, Connecticut 06457.

This work was completed with support from the National Science Foundation and the Rockefeller Foundation. The assistance of Franz Samelson, Karl Scheibe, and the staff of the Manuscript and Archive Section, Sterling Library, is gratefully appreciated.

<sup>1</sup> "Yale's Institute of Human Relations," *Yale Alumni Weekly*, 1929, 38:889–891, quoting James Angell on p. 890; "Yale is Now to Study Mankind," *New York Times*, 24 Feb. 1929; and "Yale University to Vitalize Study," *New York Times*, 14 Dec. 1930.

<sup>2</sup> For perspectives on the intellectual climate in the United States, see Thomas L. Haskell, *Emergence of a Professional Social Science: The American Social Science Association and the Nineteenth-Century Crisis of Authority* (Urbana: Univ. Illinois Press, 1977); Bruce Kuklick, *The Rise of American Philosophy: Cambridge, Massachusetts, 1860–1930* (New Haven, Conn.: Yale Univ. Press, 1977); and Dorothy Ross, "The Development of the Social Sciences," in *The Organization of Knowledge in Modern America, 1860–1920*, ed. Alexandra Oleson and John Voss (Baltimore: Johns Hopkins Univ. Press, 1979).

Whether judged by its productivity, the eminence of its participants, or its influences on later research, the IHR experiment succeeded.<sup>3</sup> However, the benefits went primarily to the field of psychology, where the researchers' work culminated in a formal and mathematical learning theory. Even by the end of the Institute's first decade the effort to implement its inaugural ideals had been replaced by a search for universal and mechanical laws of individual behavior. The failure to realize an integrated human science resulted neither from unexpected discoveries nor from political expediencies. Rather, the transformation ensued from several unanticipated consequences of the new thinking. In practice the newer conceptions of science posed a ticklish problem: scientists whose putative subject matter was the fallible and subjective human being confronted their own human limitations. The human scientists at the IHR escaped these perplexities (though to do so they had to forfeit certain goals) by rejuvenating the older intellectual world view. They also countered their reflective self-doubts with commonsense postulates about a rational social order and the place of the individual in that order; they applied these postulates to themselves as well as to their subjects. In the end the certainty that had been sacrificed in the newer models for knowledge was (re)located in an orderly methodology—in the procedural rules for conducting scientific work. Psychologists became seminal figures in the Institute, and they confronted most directly the problem of warranting knowledge claims in light of the emerging image of the nonrational and nonautonomous mind. Thus, it is not surprising that those who came to chart the course of orderly methodology were mainly psychologists.

A coherent understanding of the IHR, then, requires mapping the reciprocal relations between the shifting knowledge claims and researchers' self-assessments and social practices. This account traces the context and the occasional circularity of the relationship by examining how social practices and beliefs both informed and were informed by intellectual positions. In turn, this perspective illuminates some linkages between seemingly contradictory intellectual postures taken during the first half of the twentieth century: the construction of formalist and reductionist theory in the shadow of functionalist and pragmatic rhetoric, and the strong assertions of the rationality of scientific endeavors alongside implicative claims of human irrationality. These connections are revealed in the researchers', particularly the psychologists', attempts to organize first themselves and then their work to achieve the rational control of human behavior.

#### CONVERGING PLANS FOR HUMAN SCIENCE: PSYCHOLOGY'S SPECIAL SITUATION

By 1900 the human sciences no longer staunchly endorsed the positivism that had prevailed during the previous century. The influence of Darwin and evolutionary theory, followed closely by Freud and theories of the unconscious, suggested a

<sup>3</sup> The IHR produced dozens of monographs and hundreds of experimental reports; these studies influenced research in the following decades in such areas as aggression, socialization, learning, psychopathology, culture and personality, and motivation. A stellar list of intellectuals participated in the Institute at one time or another. In addition to those mentioned in this paper, participants included B. Malinowski, E. W. Bakke, F. B. Fitch, J. F. Fulton, and J. W. M. Whiting. The list of psychologists who became leaders in American research includes I. L. Child, J. Dollard, L. Doob, E. Erikson, E. R. Hilgard, C. Hovland, G. Mandler, D. Marquis, N. Miller, C. C. Miles, W. Miles, O. H. Mowrer, R. R. Sears, and K. Spence.

more complex reality than could be identified through a monocular positivist lens. Newer theories of human nature would have to expunge self-reliance, moral sentiments, and individual autonomy; they required a conception of remote causation that firmly distinguished epiphenomena from underlying reality. Intellectuals, confronting the dynamics, complexity, and interdependence of social life, had to find a way to assert the feasibility of comprehending that buzzing confusion of human actions, including their own.<sup>4</sup> Grounded in evolutionary theory, the functionalist idea of individual adaptations to a continually changing environment offered a coherent model for penetrating beyond proximate causes, for grasping dynamic action rather than static structure, and for seeing complex connectedness rather than unilinear causation. For some, notably psychologists, the subsequent move from functionalism to behaviorism was relatively uncomplicated; many economists, political scientists, and sociologists endorsed behaviorism as well. Quite in keeping with these trends, the ideal research plans these workers advanced often contained concrete measures for amalgamating the disciplines into an integrated human science.<sup>5</sup>

Psychologists were particularly implicated in these intellectual developments, for their studies ostensibly penetrated beyond proximate causes and into the complex dynamics of human action. They also encountered most directly the problems of rationality in science as well as in everyday life, problems that simply were not addressed by the newer world view (which, therefore, left the way open for a reassertion of positivist epistemology). These concerns emerged not only in intellectual discourse but in the questions of organization and identity psychologists entertained after World War I.

If the problems tackled by psychologists in the decades before the war had consisted of distinguishing themselves from philosophers, on the one hand, and from physiologists, on the other, and in demonstrating the practical virtues of psychological science, their war experience brought them considerable success. At the Armistice psychologists received other benefits besides their own impression of committed service: the creation of an environment, albeit temporary, for working collectively; the establishment of enduring contacts with government and industry; and a heightened public image.<sup>6</sup> As James McKeen Cattell

<sup>4</sup> For an excellent account of the transition in social scientific precepts, see Haskell, *Emergence of a Professional Social Science* (cit. n. 2). R. Jackson Wilson views the transition as a problem of reconciling the ideals of the individual and community in his *In Quest of Community: Social Philosophy in the United States, 1860–1920* (New York: Wiley, 1963).

<sup>5</sup> Appeals for integrating the social sciences often identified psychology as the key discipline for linking the others. Dorothy Ross has convincingly argued that the social sciences' moves toward scientism bolstered their legitimacy as useful instruments in society and that those toward psychology, in particular, were reformist rather than reductionist because they delineated problems as psychological rather than as biologically fixed: "Development of the Social Sciences" (cit. n. 2). Robert Church suggests a similar use of scientism in economics: "Economists as Experts: The Rise of an Academic Profession in the United States, 1870–1920," in *The University in Society*, ed. Laurence Stone (Princeton, N.J.: Princeton Univ. Press, 1974), Vol. II. See also Martin Blumer, "The Early Institutional Establishment of Social Science Research: The Local Community Research Committee at the University of Chicago, 1923–30," *Minerva*, 1980, 18:51–81.

<sup>6</sup> For varied accounts of postwar developments in psychology see John M. O'Donnell, "The Origin of Behaviorism: American Psychology, 1870–1920" (Ph.D. diss., Univ. Pennsylvania, 1979); O'Donnell, "Putting Psychology on the Map: Ideology and Intelligence Testing," in *Psychology in Social Context*, ed. Allan R. Buss (New York: Irvington, 1979); Michael M. Sokal, "James McKeen Cattell and American Psychology in the 1920s," in *Explorations in the History of Psychology in the United States*, ed. Josef Brožek (Lewisburg, Pa.: Bucknell Univ. Press, 1984); and Thomas M. Camfield,

remarked, the war involvements “put psychology on the map and on the front page.” In their triumphant return to civilian life psychologists exuded confidence—holding out for the most appealing positions, expanding applied psychology, and undertaking commercial ventures like the Psychological Corporation.<sup>7</sup>

Psychologists did confront difficulties of a different sort. Feelings of disunity were being expressed among the ranks. Many lamented the “fragmentation” of the discipline and the absence of accepted theories, methods, and subject matter; they voiced distress about the fractionalizing apparent in the competing “schools” and “systems” of psychology. The postwar committees established to consider the future of psychological research reported these structural problems. A Carnegie Institute committee set up to explore directions for research on human behavior repeatedly noted the “lack of systematic sustained and fundamental” research; the National Research Council (NRC) committee on sex problems found the state of research to be “chaotic” and “sporadic.” But in textbook accounts, critical essays, and committee reports alike, the alarms over a ruptured science were matched by a confidence not merely in the latent possibility but in the imminent emergence of a unified scientific psychology.<sup>8</sup> Prescient psychologists wrote histories that concurrently charted the dissolution of frivolous metaphysics and the appearance of superordinate scientific gains—the amorphous “harmonizing process” that was to transform American psychology.<sup>9</sup>

Hand in hand with this faith in solidarity came practical suggestions for unification. What was sought was not merely better work opportunities but organized research. Raymond Dodge asserted that what the country needed was “not a research professorship or even half a dozen of them, but a research faculty or two.” Herbert Langfeld confessed that his life goal was not to make ground-breaking discoveries but to “develop a thoroughly scientific laboratory” and “a well organized system of instruction leading to research.” Both the NRC and Carnegie committees called for coordinated and organized research in accordance with models drawn from corporate practice: the duties of the researcher, they suggested, resembled those of an executive serving his company. Edward Thorndike thought it fair to ask “scientific men to work in the spirit of the busi-

---

“Psychologists at War: The History of American Psychology and the First World War” (Ph.D. diss., Univ. Texas at Austin, 1969), p. 257. See also Daniel J. Kevles, “Testing the Army’s Intelligence: Psychologists and the Military in World War I,” *Journal of American History*, 1968, 55:565–581.

<sup>7</sup> James McKeen Cattell, “The First Year of the Psychological Corporation,” Report of the Annual Meeting, Dec. 1922, p. 5, Robert Yerkes Papers, Sterling Library, Yale University, New Haven, Connecticut. For a history of several commercial ventures see Michael M. Sokal, “The Origins of the Psychological Corporation,” *Journal of the History of the Behavioral Sciences*, 1981, 17:54–67; and Sokal, “American Psychology in the 1920s” (cit. n. 6).

<sup>8</sup> “Memoranda of Carnegie Committee for Research on Human Behavior, 1921–22”; and “National Research Committee for Research on Problems of Sex, First Annual Report, 1921–1922,” Yerkes Papers. The portentous statements on the real scientific psychology were not based on discoveries or new data but were purely anticipatory accounts. See Edna Heidbreder, *Seven Psychologies* (New York: Century, 1933); Robert S. Woodworth, *Contemporary Schools of Psychology* (New York: Ronald Press, 1931); B. H. Bode, “What Is Psychology?” *Psychological Review*, 1922, 29:252–258; Knight Dunlap, “The Outlook for Psychology,” *Science*, 1929, 69:201–207; Joseph Jastrow, “The Reconstruction of Psychology,” *Psychol. Rev.*, 1927, 34:169–195; W. B. Pillsbury, “Suggestions for a Compromise of Existing Controversies in Psychology,” *Psychol. Rev.*, 1922, 29:259–266; and Raymond Wheeler, “Outline of a System of Psychology,” *Psychol. Rev.*, 1923, 30:152–163.

<sup>9</sup> Edwin G. Boring, *A History of Experimental Psychology* (New York: Century, 1929); and Gardner Murphy, *An Historical Introduction to Modern Psychology* (New York: Harcourt, Brace, 1929). The quotation is from Woodworth, *Contemporary Schools of Psychology*, pp. 217–218.

ness employee or contractor in certain cases, and to submit to certain irritations for the probable common good."<sup>10</sup> To Harvard-trained psychologists like Robert Yerkes, cooperative organization exemplified Josiah Royce's principle of the "fecundity of aggregation." Plans based on such corporate models inevitably carried the corresponding risks of entrepreneurship. Yet the Carnegie committee saw its cooperative plan as "boldly conceived and ambitiously launched with 'definite beginnings' but with 'brave endings' in view."<sup>11</sup>

Along with the internal issue of unity, psychologists confronted an external problem: that of psychology's public image. Historians of the 1920s have viewed the popularity of psychology as a "national mania" that reinforced a common pastime of narcissistic self-analysis. But in that decade of contradictions, a time of cynicism and pessimism as well as jazz and social liberation, psychology was also the subject of jest and criticism. Especially among journalists and intellectuals, disenchantment surfaced in biting criticism of "advances" such as mental testing and behaviorism.<sup>12</sup> The challenges to psychologists' expert knowledge were compounded by the considerable public attention and support gained by psychoanalysis.

Psychologists reacted in various ways to these conditions outside the profession. Mental testers took to the popular press to defend testing. Critics of psychoanalysis described not just the faulty reasoning but the adverse psychological consequences of taking Freud seriously. Behind the public rebuttals was a more comprehensive defense of scientific psychology that continued prewar progressive arguments for social reform while augmenting the discipline's legitimacy. Arguments for the reconstruction of psychology played on images of impending social disorder. The social problems were various—growing divorce rates, delinquency, job dissatisfaction, immigrant assimilation, personality disorders, and culture lag caused by the rapid growth of science and technology and the retarded social accommodation to that growth. A reconstructed scientific psychology, particularly a social psychology, was held to be instrumental for attaining democratic social order and control. The task was typically formulated in the

<sup>10</sup> Raymond Dodge to Robert Yerkes, 31 July 1921, Yerkes Papers (Dodge, who criticized John Watson for a self-interestedness that made him "blind to everything beyond his narrow, self-imposed horizon," clearly believed that such antisocial behavior was detrimental to psychology); Langfeld to Yerkes, 20 May 1924, Yerkes Papers; and Edward Thorndike, "Human Behavior: A Plan for Aiding the Study of the Human Individual after Infancy, to the End of Discovering Fundamental Fact and Laws of Human Nature and Behavior," submitted to the Carnegie Committee, 1921, p. 7, Yerkes Papers. See also James McKeen Cattell, "Early Psychological Laboratories," *Science*, 1928, 67:543–548. If one heeds the conversations among social scientists on the prescribed corporate research models, it appears that it was not the business world that would benefit from the social sciences in the 1920s but the converse. Corporate management models are evident even in discussions about the creation of the Social Science Research Council: see Barry D. Karl, *Charles E. Merriam and the Study of Politics* (Chicago: Univ. Chicago Press, 1974).

<sup>11</sup> Yerkes to Abraham Flexner, 15 Jan. 1919, Yerkes Papers; and "Report of Second Conference on Plans for Extension of Biological Research," held by Carnegie Institute, 28 Jan. 1922, pp. 2–3, Yerkes Papers.

<sup>12</sup> On the 1920s, see, e.g., William E. Leuchtenburg, *The Perils of Prosperity, 1914–1932* (Chicago: Univ. Chicago Press, 1955), p. 164; and Paula Fass, *The Damned and the Beautiful: American Youth in the 1920's* (New York: Oxford Univ. Press, 1977). For contemporary criticisms of psychology, see Stephen Leacock, "A Manual for the New Mentality," *Harper's*, 1924, 148:472–480; Almy Rodgers Walker, "What Is Your Garden Complex?" *Harper's*, 1928, 157:255–256; G. H. Estabrook, "Tell It to the Traffic Cop," *Harper's*, 1928, 157:277–279; and Grace Adams, "The Rise and Fall of Psychology," *Atlantic Monthly*, 1934, 153:82–92. See also Walter Lippmann to Yerkes, 9 Jan. 1923, Yerkes Papers.

functionalist terms of individual adjustment and adaptation to a changing environment. Given the purported culture lag and an etiology locating these problems at the individual level, psychology became the obvious source for acquiring the necessary rational knowledge.<sup>13</sup>

Psychologists' self-designated role had roots in prewar reform sentiments, but during the 1920s even greater emphasis was placed on psychological expertise and, accordingly, on the fallibility of common sense. The argument in favor of guiding individual adjustment to social institutions was bolstered by the claim that ordinary reasoning was inferior to rational scientific thought. Thus John B. Watson talked about common sense as "crude" psychology. Edward Thorndike warned of the permanent limitations of the ordinary being or "half-educated man," and Max Meyer wrote a volume on the irrational psychology of "the other one."<sup>14</sup> But however vehement the proclamations of their superior rationality, psychologists, unlike physical scientists, self-consciously confronted their own personal and cognitive limitations. The distinctions between the psychologist and the person in the street were not yet so durably cemented. Hence Edwin G. Boring's 1928 presidential address to the American Psychological Association, in dealing with the pertinent issue of intellectual fragmentation and controversies, concentrated on the unappealing personal and emotional components of psychologists' work. Raymond Dodge vividly described the "perils" of the psychological researcher who "may get lost in the chaos of details and never emerge. I have known such lost souls." The researcher "may find himself in conflict with his colleagues or with the native inhabitants of the dark continent of ignorance, who voluntarily choose darkness rather than light and prefer prejudice to information. Not all of them live in Tennessee." Even Watson, who was perhaps the most arrogant in his scientific claims, confessed privately to occasional feelings of hopelessness in his quest to understand himself and others.<sup>15</sup>

By the 1940s these self-doubts and uncertainties were worked out, primarily

<sup>13</sup> Social control and transformation of the backward laity by adjusting individual behavior became a common goal. The person in the street was portrayed as irrational, illogical, mentally deficient (as the Army testing program had shown), and neurotically self-involved (see n. 12). Many psychologists shared F. S. C. Schiller's hope that psychology might "invert the miracle of Circe and transform the Yahoo into a Man": *Tantalus, or the Future of Man* (London: Kegan Paul, Trench, & Trubner, 1924), p. 64. The objectives of social control are explored in John Burnham, "The New Psychology: From Narcissism to Social Control," in *Change and Continuity in Twentieth-Century America: The Nineteen-Twenties*, eds. J. Braeman, R. H. Bremner, and D. Brody (Columbus: Ohio State Univ. Press, 1968); and J. G. Morawski, "Assessing Psychology's Moral Heritage through Our Neglected Utopias," *American Psychologist*, 1981, 37:1081-1095.

<sup>14</sup> John B. Watson, *Psychology from the Standpoint of a Behaviorist* (Philadelphia: Lippincott, 1919), p. 7; Edward L. Thorndike, "The Psychology of the Half-Educated Man," *Harper's*, 1920, 140:666-670; and Max Meyer, *The Psychology of the Other-One* (Columbia: Missouri Book, 1921). See also n. 13. The claims for psychologists' superior vantage point actually helped practitioners reach consensus on just what this special vantage point was. It also meant that they had special expertise in making decisions about social life: see, e.g., Edward L. Thorndike, "Psychology of the Half-Educated Man," p. 670. Elsewhere I have suggested how such privileging of knowledge functioned in securing professional legitimacy for psychology: J. G. Morawski, "Psychology and the Shaping of Policy," *Berkshire Review*, 1983, 18:92-107; and Morawski, "The Measurement of Masculinity and Femininity: Engendering Categorical Realities," *Journal of Personality*, 1985, 53:196-223.

<sup>15</sup> Edwin G. Boring, "The Psychology of Controversy," *Psychol. Rev.*, 1929, 36:97-121; Raymond Dodge, "Excursions in Experimental Psychology," *Scientific Monthly*, 1926, 23:129-137, on p. 129; and (e.g.) Watson to Yerkes, 7 Feb. 1916, Yerkes Papers. Watson's bouts of self-doubt are rare but poignant punctuations in his personal correspondence, and they contrast sharply with his self-assured public voice.

through "logical" methodologies and discrimination between the rational capacities of the "experimenter" and the "subject." But the steps toward managing such doubts entailed the validation of certain scientific practices. In a call for "centralized" planning, Angell voiced the most common recommendation for structuring research: research programs that were corporate and cooperative in nature would ensure against the cognitive idiosyncrasies of individual scientists. He and others challenged the cognitive competency of the individual and derided the individualistic "fetish" among researchers, though they usually appended an exception for an anomaly, the "genius."<sup>16</sup> Organized and cooperative research was an initial answer to the inherent irrationality of the individual mind as well as to psychology's problem of a house divided against itself. Thus, while physical scientists were publicizing their postwar enterprise as truly social and democratic in an effort to dismantle their elitist public image, psychologists were promoting the same arrangements as a means of exhibiting communality, rationality, and self-assurance.<sup>17</sup>

#### RECONSTRUCTION AT YALE

Robert Yerkes was among those who sought new schemes for psychology and who proposed that an organized institute be established at Yale. Yerkes's research program adhered to the conceptions of human nature as they took form in the "new" and eclectic psychology, particularly functionalism, behaviorism, and comparative (animal) studies.<sup>18</sup> His employment history typifies the career pattern of the new psychology. After the war Yerkes remained in Washington at the National Research Council, although during those five years of bureaucratic chores he actively sought support for psychological research. Among his desires

<sup>16</sup> James Angell, "The Organization of Research," *Sci. Monthly*, 1920, 11:25–42, on p. 35. Angell repeatedly proclaimed the urgent need for organized research on human problems, arguing both that researchers needed guidance devices and that the average individual is prejudiced, impulsive, uninformed, biased by custom, and conservative. To many writing on this theme the "genius" characteristics of the scientist were scarcely, if at all, distinguishable from abnormal personality traits. Such characteristics were at once venerated and abhorred. In any case, they required control. See Angell, "The Evolution of Intelligence," in *The Evolution of Man*, ed. George A. Baitsell (New Haven, Conn.: Yale Univ. Press, 1922); Angell, "The Development of Research in the United States," *Reprint and Circular Series of the National Research Council*, 1919, 6:1–20; and Leo E. Savidla and W. E. Gibbs, eds., *Science and the Scientific Mind* (New York: McGraw-Hill, 1930). For the stance of the American Psychological Association, see Samuel W. Fernberger, "The American Psychological Association: A Historical Summary, 1892–1930," *Psychological Bulletin*, 1932, 29:1–80, on p. 50.

<sup>17</sup> Attempts to devise a cosmology linking social science and American social life, and with that an argument for supporting fundamental research, resembled the concurrent efforts of physical scientists. However, there are significant differences between the claims of the two groups. Whereas physical scientists attempted to depict the basically social and democratic nature of scientific practice in order to allay negative public perceptions, psychologists stressed the essential differences between the everyday social world of ordinary reasoning and scientific practice. Psychologists considered organized research to be the solution to a fragmented knowledge base, while physical scientists saw organization simply as necessary to large-scale endeavors: see Ronald C. Tobey, *The American Ideology of National Science, 1919–1930* (Pittsburgh: Univ. Pittsburgh Press, 1971).

<sup>18</sup> A strong commitment to evolutionary naturalism is revealed in most of Yerkes's research: in his "psychobiology," eugenics, mental testing, and comparative psychology. Nevertheless, he held that introspection was indispensable to psychology: see Robert Yerkes, "Comparative Psychology: A Question of Definitions," *Journal of Philosophy, Psychology, and the Scientific Method*, 1913, 10:580–582; Yerkes, "The Study of Human Behavior," *Science*, 1914, 4:176–184; Yerkes, "Eugenic Bearing of Measurements of Intelligence in the United States," *Eugenics Review*, 1923, 14:223–245; Yerkes, *Almost Human* (New York: Century, 1925); and Yerkes, "Private Cooperation and Intelligence," *American Journal of Psychology*, 1937, 50:254–270.

was to establish an integrated research center—an “ape farm”—and he even contemplated setting up a joint ape and infant research station with Watson.<sup>19</sup>

In 1922 Yerkes approached Angell, the newly appointed president of Yale, with a proposal for a “combined research and teaching institution.”<sup>20</sup> Two years later the Laura Spelman Rockefeller Memorial granted the Yale plan a five-year trial funding at forty thousand dollars per year. Yerkes’s idea of a centralized research station was well received, and the management combination of Angell, Yerkes, and the Memorial was fertile. At the turn of the century Angell had been one of the University of Chicago’s foremost proponents of functional psychology. Through his administrative experience during the war and then as president of the Carnegie Corporation he had developed functionalist arguments for creating cooperative, well-managed, and generously endowed research programs in the human sciences.<sup>21</sup> When Angell arrived at Yale in 1921, the quality of the graduate school was unimpressive. Psychology in particular was in a quagmire: it was still part of the philosophy department, had been responsible for no major research, and had produced only eight Ph.D.s between 1903 and 1921 (compared with Clark’s seventy-five, Chicago’s fifty-one, and Columbia’s forty-six). Despite his rigorous pursuit of various university reforms and funding campaigns, Angell had apparently done nothing to remedy these problems in psychology when Yerkes approached him.<sup>22</sup>

The contacts with the Rockefeller Memorial were also fruitful, for the young foundation was interested in supporting social research and in 1922 had hired the psychologist Beardsley Ruml as its president. Ruml had studied at Chicago when Angell was there, had been associated with Yerkes and Angell through psychologists’ war work, and had been Angell’s assistant at the Carnegie Corporation.<sup>23</sup>

<sup>19</sup> Yerkes to Watson, 14 May 1913, 8 June 1913, 29 Nov. 1916, and 29 Mar. 1919, Yerkes Papers. No mention of this cooperative project is made after Watson’s abrupt departure from academic life in 1920. Yerkes sometimes claimed that such research centers were crucial to preserving society: Robert Yerkes, “Progress and Peace,” *Sci. Monthly*, 1915, 2:195–201; and Yerkes, “The Road of Psychology,” *Open Road*, 1922, 4:56–63.

<sup>20</sup> Yerkes to Angell, 11 June 1922, Yerkes Papers.

<sup>21</sup> Angell’s most popular work on functionalism and behaviorism is his textbook, *Psychology: An Introductory Study of the Structure and Functions of Human Consciousness* (New York: Holt, 1904). See also Angell, “The Province of Functional Psychology,” *Psychol. Rev.*, 1907, 14:61–91; Angell, “The Influence of Darwin on Psychology,” *Psychol. Rev.*, 1909, 16:152–169; Angell, “Behavior as a Category of Psychology,” *Psychol. Rev.*, 1913, 20:255–270; Angell, “Organization of Research” (cit. n. 14); and Angell, “Development of Research” (cit. n. 16).

<sup>22</sup> During Angell’s tenure as president (1921–1937) the university endowment grew from \$25 to \$107 million, over \$60 million had been spent on building, the undergraduate enrollment had doubled, undergraduate aid had quadrupled, and graduate student support had been multiplied many times that: George W. Pierson, *Yale: College and University, 1871–1937* (New Haven, Conn.: Yale Univ. Press, 1955), Vol. II, p. 521; Brooks M. Kelley, *Yale, A History* (New Haven, Conn.: Yale Univ. Press, 1974), pp. 276–278, 330; and Robert S. Harper, “Tables of American Doctorates in Psychology,” *Amer. J. Psychol.*, 1949, 62:580–582.

<sup>23</sup> Ruml’s relationship with Angell was amicable, and in correspondence he called Angell “Chief.” Ruml also supported both the 1924 and 1928 plans from Yale: see Laura Spelman Rockefeller Memorial Archives, Series 3, Box 79, Rockefeller Archives Center, North Tarrytown, New York. While head of the Memorial, Ruml argued for developing organized practical social science research: see Beardsley Ruml, “Reconstruction in Mental Tests,” *Journal of Philosophy*, 1921, 18:181–185; and Ruml, “Recent Trends in Social Science,” in *The New Social Science*, ed. Leonard A. White (Chicago: Univ. Chicago Press, 1930), pp. 99–111. In 1930 Ruml declined Angell’s invitation to join the Yale faculty, choosing instead the informal and familiar University of Chicago: Ruml to Angell, 18 Dec. 1930; Angell to Ruml, 22 Dec. 1930, Presidential Papers of James Rowland Angell, Sterling Library, Yale University.



The Institute of Psychology opened in 1924; it was staffed by the psychologists Yerkes, Raymond Dodge, and Roswell Angier, the last from Yale's psychology group, and Clark Wissler, an anthropologist. They established their own research programs and were responsible only for training graduate students. The reality of the Institute fell somewhat short of what was planned; the participants in this organization for cooperation and integration, in addition to Yerkes in genetic and comparative psychology and Dodge in general psycho-physiological psychology, were to have been Karl Lashley for neurology and abnormal psychology, Carl Brigham for mental measurement and statistics, and Edwin Boring for theory and history. The failure to secure the ideal personnel matrix did not hamper productivity. However, the Institute proved to be simply a high-powered psychology department where members, exonerated from undergraduate teaching and equipped with new laboratories, were free to pursue their independent research projects.<sup>24</sup>

Before the "experiment" that was the Institute of Psychology underwent its five-year review, new plans practically left that center abandoned in the shadows. While Yerkes was stocking his primate laboratory and the Institute of Psychology was continuing its search for a mental tester (Clark Hull was finally hired in 1929), the talk around New Haven concerned a research center especially designed for "human behavior" studies. The aims of the proposed center resembled those of the Institute of Psychology—organized, cooperative, and integrative research—but were much grander in proportion. In considerable part, these schemes were devised by Robert Hutchins, then dean of the law school, and Milton Winternitz, dean of the medical school. Committed to the improvement of professional training and to advancing the knowledge of human nature in that training, both men initiated plans for modernizing their schools. "Modernization" in this case meant incorporating the functionalist perspective on human behavior and formally organizing the resultant interdisciplinary science. Both deans perceived the psychological branches of human science to be crucial for the practice of law and medicine. For some years Winternitz had been preoccupied with the medical profession's neglect of the human mind; the inadequacies were particularly evident at Yale, where, he believed, psychiatry had not merely lagged but "remained stationary." He regretted the passing of the general practitioner, whom he admired as an "empirical psychiatrist," and argued that specialization had attended to all other functional units "at the expense of the mind." Winternitz also sought "preventive medicine" against the circumstances of metropolitan life, which overtaxed the entire human system, and advocated remedial procedures that would adjust individuals to their occupations and environment.<sup>25</sup>

<sup>24</sup> "Statement Concerning Plan and Objects of the Institute of Psychology at Yale"; and "First Annual Report of the Institute of Psychology, 1925," Yerkes Papers. See also files of the Institute of Psychology in Angell Presidential Papers and Yerkes Papers. Despite several approaches, Brigham declined the Yale invitation, and it appears that Karl Lashley and Edwin Boring were never approached.

<sup>25</sup> Winternitz, "Report of the Dean, School of Medicine, 1922–1923," p. 25; and Winternitz, "Report of the Dean, School of Medicine, 1921–1922," pp. 4, 27, 29; Milton C. Winternitz Papers, Sterling Library, Yale University. See also Milton C. Winternitz, "Medicine and the Social Sciences," in *New Social Science*, ed. White (cit. n. 23), pp. 40–45; Winternitz, "The Institute of Human Relations at Yale," *New England Journal of Medicine*, 1930, 202:57–59; Winternitz, "The Human Welfare Group," *Yale Alumni Weekly*, 1930, 39:683, 686–687, 689–690; Winternitz, "A New Educational Pattern," *Clinical Medicine and Surgery*, 1931, 38:473–550; and Winternitz, "A Physician Looks at Mental Hygiene," *Mental Hygiene*, 1932, 16:221–232.

Although Adolf Meyer, a psychiatrist at Johns Hopkins, advised against it, Winternitz proposed to teach “behavior” in the medical curriculum as well as to ensure the organized interaction of research psychologists, psychiatrists, and neurologists.<sup>26</sup> Meanwhile, the twenty-eight-year-old law school dean contemplated making American legal studies truly scientific, even experimental, and allying it with general human behavior studies. Writing in the journals of both fields, Hutchins advocated the alliance between law and psychology as a basis for legal research, pedagogy and practice. However, he did insist that psychology would be relevant only if it was “interested in human behavior, instead of in epistemology and cosmology.” Psychology would offer a corrective to the fact that “the law proceeds on a common sense art of behavior, which fails to hold its ground in the face of scientific data.”<sup>27</sup>

After independently preparing a handful of proposals, Winternitz and Hutchins combined their visions for an integrated research center in a proposal that Angell presented to the Rockefeller Foundation (whose Division of Social Sciences had succeeded the Rockefeller Memorial) in 1928. The result was plans for the Human Welfare Group, an elaborate network connecting those graduate and professional schools that dealt with the human sciences in any way. In fact, the arrangement was so elaborate that several descriptive pamphlets, one with twelve questions and answers about the Human Welfare Group, were published, along with a multipage flow chart linking the various schools, clinics, and departments (see Figure 1).<sup>28</sup> In the upper center of the chart, singularly placed in the middle of a star, was the Institute of Human Relations. While the Human Welfare Group was to deal with “the body of man in health and sickness, the mind of man as it appears normally and pathologically, and the relations of man to others and to his environment,” the IHR had special responsibilities. To it was accorded the undertaking of systematic research to correlate knowledge from the various scientific branches. The IHR would strike down the superficial disciplinary boundaries and show how man is “a composite made up of three elements—mind, body, and environment—in constant interaction and impossible of separation. Not one of these elements has in itself any reality,” for they exist only in relation to one another.<sup>29</sup> The IHR, then, would both accommodate and expand the functions of the Institute of Psychology.

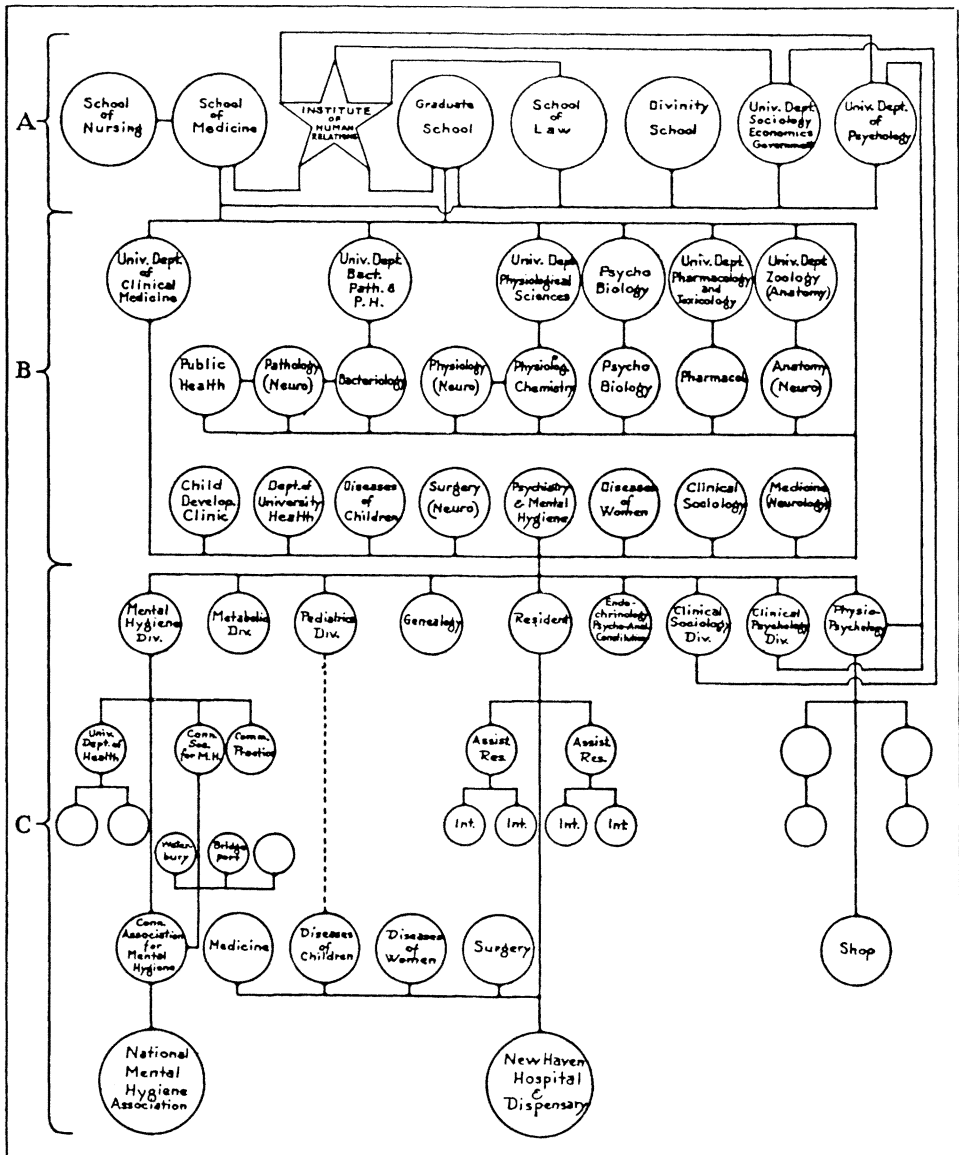
The massive project was funded, primarily by the various sections of the Rockefeller Foundation, and most of the money was allocated to what seemed to

<sup>26</sup> Adolf Meyer to Winternitz, 17 Dec. 1923; Winternitz to Meyer, 3 Jan. 1924; Meyer to Winternitz, 4 Jan. 1924, Adolf Meyer Papers, Johns Hopkins School of Medicine, Baltimore, Maryland.

<sup>27</sup> Robert Hutchins and Donald Slesinger, “Legal Psychology,” *Psychol. Rev.*, 1929, 36:17–26. See also Hutchins and Slesinger, “Some Observations on the Law of Evidence: The Competency of Witnesses,” *Yale Law Journal*, 1928, 37:1017–1028; Hutchins, “The Law School Tomorrow,” *North American Review*, 1928, 15:129–140; Hutchins, “The Yale Law School in 1928,” *Connecticut Bar Journal*, 1928, 2:1–9; Hutchins, “The Law and the Psychologists,” *Yale Review*, 1927, 16:678–690.

<sup>28</sup> “A Program for an Institute of Human Relations at Yale University,” 20 May 1928; Angell to Edmund Day, 20 Dec. 1928, Laura Spelman Rockefeller Memorial Archives. For the pamphlets, see *Yale Proposes to Study Man* (New Haven, Conn.: Human Welfare Group, 1929); *The Human Welfare Group, New Haven, Connecticut: For the Promotion of Health, Physical and Mental, Individual and Social* (New Haven, Conn.: General Hospital Society, Yale Univ., 1929); *12 Questions about the Human Welfare Group* (New Haven, 1929). The chart, which shows the IHR as the “activating center and correlating agent” of the program, is in the Archives of the Institute of Human Relations, Sterling Library, Yale University.

<sup>29</sup> *Human Welfare Group*, pp. 2, 4, 39–40.



**Figure 1.** Chart depicting organization of the Human Welfare Group, indicating "the manner in which related activities in the pure and applied sciences are tied together to form a unified group of which the Institute of Human Relations is the activating center and correlating agent." Courtesy of Sterling Library, Yale University.

be everyone's special concern, the IHR. The Foundation granted the IHR \$150,000 per year for the first ten years and nearly \$2 million for its building, which, constructed in the center of the medical school complex, was specially designed to accommodate laboratories for studying human and infrahuman behavior. With only a slight alteration in title to delete the word "behavior" (one dean claimed that the word was too Freudian for Yale), a center for organizing

the study of human nature was realized and the "experiment in total science" commenced.<sup>30</sup>

### EXPERIMENTAL PLANS

Adolf Meyer graciously accepted the invitation to be one of the fifty members of the National Advisory Committee of the Human Welfare Group, but he immediately wrote to the president of Johns Hopkins University, Joseph Ames, about the foolishness of Yale's program. He called the IHR "the dream of planners, not of workers," and recommended that Johns Hopkins find the real workers.<sup>31</sup> The Institute in fact enforced no clearly demarcated division of labor; at the ten-year review, the lack of such division both confused and worried Rockefeller officials. The changes in organization and research strategies that occurred during that first decade appear almost as bootstrap efforts to demarcate the blurred goals of cooperation among workers, integration of theory, and practical application.

The transitions in organization, from individually initiated projects and disciplinary autonomy to integrated research units and regular seminars, actually parallel changes in the working theories of human nature at the Institute. The participants strove for a more efficacious corporate and cooperative structure while at the same time moving toward a more mechanical and formal conception of human action. As they did, the workings of their self-regulated organization more closely approximated the "control" and "order" they hoped their scientific discoveries would bring to the larger society. These experiments in manageable social arrangements reveal the psychological resemblances perceived between the scientist and the nonscientist. Consideration of the ever-present specter of irrationalism, especially through discussions of Freudian theory, further highlighted the resemblances. Thus, as the IHR participants constructed their more orderly research organization and scientific theory, they became increasingly attentive to strategies that would reduce irrationality and validate their work as *rational*.

The original IHR members were twenty-one Yale faculty members, invited by President Angell and the Institute's Executive Committee, who were to give an appreciable part, if not all, of their time to Institute work. In addition to the five members of the former Institute of Psychology and the psychologists Mark May and Arnold Gesell, the list of those invited included individuals from law, economics, history, medicine, sociology, political science, and psychiatry. During the first two years a number of appointments to the Yale faculty intended to fortify the Institute's expertise were made; these included W. O. Douglas (law), Dorothy Thomas (sociology and law), Walter Miles and Catherine Cox Miles (psychiatry and psychology), and Edward Sapir (anthropology). Donald Slesinger, a sociologist hired two years earlier for his interdisciplinary skills, coordinated the five-member Executive Committee. When Slesinger followed Hutchins to Chicago in 1930, Winternitz and the dean of graduate studies, Edgar Furniss,

<sup>30</sup> Elliott Dunlap Smith, "An Experiment in Total Science Approach," undated clipping in Yerkes Papers.

<sup>31</sup> Meyer to Ames, 22 Oct. 1929, Meyer Papers. Meyer erroneously referred to his being invited to advise the IHR activities, whereas in fact he was asked to join the Human Welfare Group National Advisory Committee.

became associate directors. This administrative system lasted until 1935, when Mark May was appointed director; he remained in the position until the final years of the IHR.

Research funds were allocated by individual departments, and the Executive Committee primarily provided advice and guidance. However, it did sponsor research projects on social problems—among them costly studies on juvenile delinquency, the assimilation of immigrants, automobile accidents, human relations in industry, business failures, college personnel relations, bank credits, and law enforcement. Concerning these studies, the rationale of the Executive Committee was straightforward: “In as much as the failures of man to make satisfactory adjustments to his environment constitute the most pressing problems from the social point of view and at the same time afford data not so readily available where the adjustment is satisfactory, attention is being focused in the first Institute cooperative projects upon areas in which there are such evidences of failure as disease, poverty, unemployment, and crime.”<sup>32</sup>

The same document reiterated a rationale for the *laissez-faire* management of the Institute. The IHR was to be a “symbol of that synthesis of knowledge, for which need is now so widely recognized,” which, through its physical existence and policy of a common working space, would encourage the “free intercommunication” and “informal contacts” requisite for cooperative work. In other words, it should keep researchers “constantly mindful of their common purposes.”<sup>33</sup> The original organization emphasized “voluntary” and “informal” association and assumed that coercing researchers to modify their research plans or follow a blueprint would violate their academic liberty.<sup>34</sup>

In fact, the early years yielded neither cooperative research nor integrated theory but rather produced a considerable number of studies on social problems. The explanation given for the failure to generate cooperation and integration was threefold: the distribution of funds to individuals or departments inhibited the voluntary association of scientists; the sensationalism of the applied work overshadowed the slow and subtle progress on scientific theory; and the tendency of established, mature scientists to continue working independently in accustomed ways blocked cooperation.<sup>35</sup> The simple theory that propinquity would generate collective scientific practice had to be amended. This realization led in 1932 to a proposal for systematizing all research projects and then, in the following three years, to various critical appraisals and alternative plans. The 1932 proposal, submitted by Mark May, contained a “hypothesis” about personality and social structure that would provide a theme for unifying the biological and social sciences. May’s “working theory” was a dual-aspect view in which the individual and social organization were recognized as constituting the same reality. The

<sup>32</sup> “Summary of Purposes and Activities of the I.H.R.,” 18 Nov. 1930, pp. 1–34, on p. 8, Angell Presidential Papers.

<sup>33</sup> *Ibid.*, p. 4. These claims on behalf of voluntary association had originally been made in the 1928 proposal to the Rockefeller Foundation.

<sup>34</sup> Mark May, “A Retrospective View of the Institute of Human Relations at Yale,” *Behavior Science Notes*, 1971, 6:141–172.

<sup>35</sup> Minutes of Executive Committee Meetings, March 1929–1931, Angell Presidential Papers. These criticisms were reviewed in May’s history, “Retrospective View of the IHR.” Various IHR members independently voiced these criticisms; e.g., Yerkes sent a formal memo to Angell as early as 1930 appealing for young researchers because of their “greater plasticity and open-mindedness”: memo to Angell, Oct. 1930, p. 5, Yerkes Papers.

proposal offered a systematization of specific research projects and equally explicit directives for organizing research personnel. "Specialists," with the assistance of "field workers" and "office staff," would collect data on one precisely defined aspect of the central theme, and the "general director" would take responsibility for incorporating the data into the overall system.<sup>36</sup>

Although President Angell personally approved of May's systematization, nothing concrete emerged from the proposal.<sup>37</sup> Other IHR members, although satisfied with the general working environment, were similarly discontented with the absence of cooperative work and unified theory. Raymond Dodge was the next to explore alternatives, and although he, too, embraced the idea of systematization, his model was at once less bureaucratized and more demanding than May's. Dodge argued that the Institute had multiple responsibilities of producing socially beneficial science and questioned whether these obligations would be met if left to the participating scientists. He eschewed the "piece-work method in which a director sets the problems" and the staff "become extra manipulating hands." The real question was whether a "community of aims" could be identified empirically; given that the IHR was an experiment, the experimentalists should ascertain the "facts" about the viability of such a community. Dodge's solution to the general organizational problem implied a special understanding of science: science comprises the free enterprise, or the "intellectual capital," of the individual scientist's mind, yet it simultaneously depends on group activities. The "community of aims" of free and mature investigators included a responsibility to find the thread of continuity uniting them all. In other words, it required a collective and inherently *social* "systemization."<sup>38</sup>

As they prepared for the Institute's five-year review in 1934/35, the Executive Committee also attended to the apparent disorganization. Several young researchers were hired in the belief that they would be more flexible and more willing to engage in a cooperative integrated science than the older scientists. To ensure centralized control, a full-time director, Mark May, was appointed. Funds were transferred from the inpatient psychiatric services to support the director and research assistants. This move also served to free the Institute of units that resisted integration, notably the Psychiatry and Mental Hygiene programs, Arnold Gesell's Child Guidance Clinic, and Yerkes's primate laboratory. Finally, the director was given full control of research funds.<sup>39</sup>

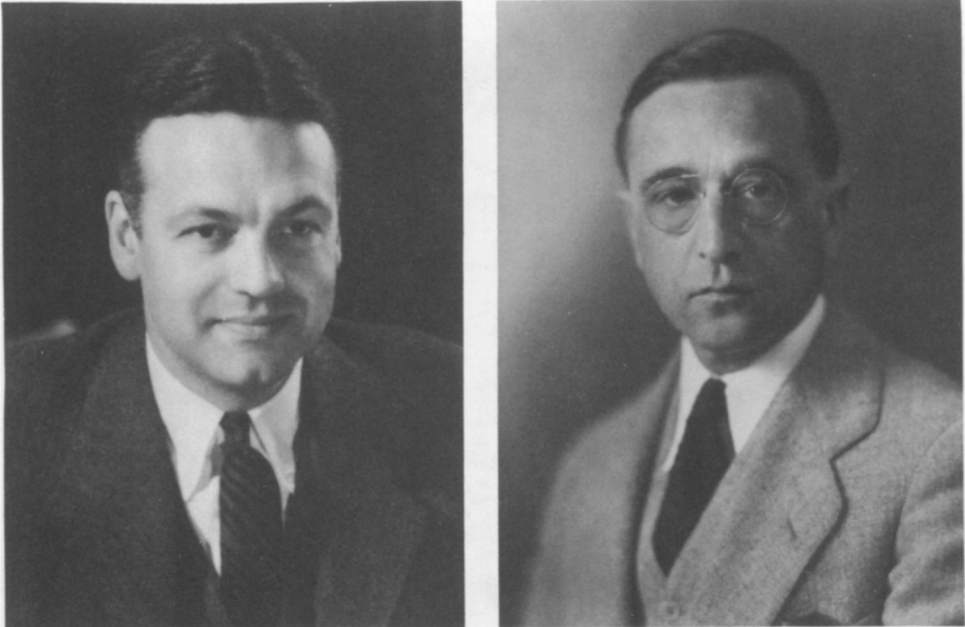
Rockefeller officials approved of these amendments, for they were disappointed by the Institute's administrative and research complications. The Foundation nevertheless continued to endorse "pure" scientific research on social and mental life as well as the idea of interdisciplinary work. The Rockefeller Foundation Director of Social Sciences, E. E. Day, wrote that the Institute still promised to contribute to "our fragmentary and frightfully inadequate knowledge of

<sup>36</sup> Mark May, "A Program of Research for the Social Science Division of the Institute of Human Relations," 1932, Angell Presidential Papers.

<sup>37</sup> Angell to May, 1 June 1932, Angell Presidential Papers.

<sup>38</sup> Raymond Dodge, "Dinner Meeting Remarks of Professor Connected with the I.H.R.," 12 May 1933, pp. 2, 4, 5.

<sup>39</sup> Memos of the Executive Committee, October–December 1931; May to Angell, 6 Dec. 1935, 21 May 1936; "Review of the IHR," 12 Apr. 1935, IHR Archives; and Mark May, "Memorandum to the President of the University," 20 Jan. 1938, Rockefeller Foundation Archives, Rockefeller Archives Center.



**Figure 2.** Robert Hutchins (left), then dean of Yale's law school, and Milton Winternitz (right), then dean of the medical school, were instrumental in organizing the Human Welfare Group and the Institute of Human Relations. Courtesy of Sterling Library.

the mental, emotional, and volitional life of man." To Rockefeller's Director of Medical Sciences, Alan Gregg, the involvement of May and the "youngsters" was heartening, because "May wants team work—'a team to shove the ball forward.'"<sup>40</sup>

The Institute members did not relax their concerns after the review. Rather, the years from 1935 to 1938 were the most active time for both policy planning and scientific theorizing. Through conferences, critiques, and proposals it became apparent that controls should be established not only over administrative activities but over research methods and theory as well. Dodge revived his plea for the systematization of research, and John Dollard criticized the communication barrier created by the dichotomies of clinical versus experimental methods and of human "behavior" versus human "relations" concepts.<sup>41</sup> May abandoned entirely any dream that the heads of units would "get together voluntarily" to do coordinated research and hoped instead that central control of funds would remedy the disunity. As he informed Alan Gregg, he imagined the IHR to be like a department but without departmental responsibilities. He wanted to develop a social science with "power" like physics but without the constrained discourse of that science. On the same day another Institute member, Clark Hull, shared his

<sup>40</sup> Day to Raymond Fosdick, "Memo on the IHR," 28 July 1936, p. 1, Rockefeller Foundation Archives; and Gregg, "Memo on Interviews at IHR, November 16–19, 1936," p. 1, Rockefeller Foundation Archives.

<sup>41</sup> Clark Hull to Robert Yerkes, 18 June 1935 (on Dodge), Yerkes Papers; Dollard to E. E. Day, 2 Feb., 27 Feb. 1935; and Alan Gregg, "Memo of Visit to IHR, October 18, 1937," Rockefeller Foundation Archives.

scenario with Gregg: coordination required leadership to change the “behavior” and “attitudes” of the researchers so the Institute could bring to psychology what quantum mechanics had brought to Newtonian ideas.<sup>42</sup> These remarks were just the beginning of Hull’s planning; he devoted substantial time to contemplating an organizational structure to implement, as well as a scientific theory to justify, these plans. Although modified by the interests of other IHR members, his plans would substantially alter the scientific procedures and products of the community.

Since being hired in 1929 as the psychotechnician of the Institute of Psychology, Hull had stood more or less backstage at the IHR performance, and his single cooperative proposal, for eugenics research, was not adopted. He pursued research in aptitudes, hypnosis, and learning and in 1935 became deeply interested in the functioning of the IHR; he believed that the Institute was an adventure that could itself make scientific history.<sup>43</sup> He developed blueprints for an efficacious administration as well as for research, and he buttressed both sets of plans with a psychological theory of scientific practice. The problem, as he presented it to Yerkes, involved reconciling the “fact of human nature” that individuals are essentially uncooperative with the ideal of a democratic community. This problem had three possible solutions: allow individuals’ (scientists’) activities to “drift along” as before and eventually lose support, cooperatively and voluntarily select a topic for coordinated investigation, or secure a “Hitler type” to coerce the members more or less violently into performing coordinated activities. Although Hull claimed to prefer democracy, his theoretical jottings and later autobiographical statements attest to a resolute belief in regimental procedures and an orderly hierarchy of authority.<sup>44</sup>

Hull’s moves to restructure the Institute entailed developing a concrete research agenda. His working blueprints for the collective study of foodseeking, hunger, frustration, and conflict in animals and humans—all of whom were to be housed in the Institute—were ultimately discarded in favor of the general subject of motivation, a topic that was being considered by other members. Hull decided to conduct a seminar on two competing motivational theories: psychoanalysis

<sup>42</sup> May, “Memorandum on the Organization of the Institute of Human Relations,” 14 Dec. 1936, Angell Presidential Papers; and Gregg, “Memo on Interviews at IHR, November 16–19, 1936” (cit. n. 40), p. 2.

<sup>43</sup> Hull’s proposal for eugenics research was made in response to Angell’s solicitation for projects on the subject, which in turn was an attempt to utilize an anonymous donation earmarked for that purpose: “Idea Book, 1927–1930,” Clark L. Hull Papers, Sterling Library, Yale University. Most notable among Hull’s chosen research ventures were hypnosis, aptitude testing, and learning theory. His efforts to attain prominence, if not superior leadership, in these areas is often noted in his ideabooks (1925–1945; Hull Papers). These notes intimate a continuity in these researches, specifically, a certain methodological orderliness and philosophy of science. Rodney G. Triplett has suggested that there is also continuity in Hull’s underlying psychological models: see Triplett, “The Relationship of Clark L. Hull’s Hypnosis Research to his Later Learning Theory: The Continuity of his Life Work,” *J. Hist. Behav. Sci.*, 1982, 18:22–31.

<sup>44</sup> Hull to Yerkes, 18 June 1935, Yerkes Papers; see also Gregg’s “Memo on Interviews at IHR, November 16–19, 1936” (cit. n. 40), p. 2. Even in proclaiming a democratic position, Hull doubted the willing participation of individuals. He suggested that May would need some “magic” for eliminating certain “characteristics of human nature” that prohibit democratic coordination. In his autobiography Hull claimed that the Institute really needed a führer, and that although democratic pretenses proscribed such arrangements, he could induce others to follow his directives: see “Clark L. Hull,” in *History of Psychology in Autobiography*, ed. E. G. Boring, Herbert S. Langfeld, Heinz Werner, and Robert M. Yerkes (Worcester, Mass.: Clark Univ. Press, 1952), Vol. IV, p. 156.



and behaviorism. Weekly meetings, attended by Institute members and dedicated to a central problem, would serve as an "integrating device" for the IHR.<sup>45</sup>

The reason for holding a seminar, especially one that posed two antagonistic theories, was not solely to ensure the propinquity or voluntary association of scientists. The idea was grounded in Hull's psychological theory of the researcher. In turn, this very psychology constituted the foundation for creating a unified science of human relations. Like many other psychologists, Hull was periodically distressed by the faulty cognitions and (social) misconduct of researchers. As to the first, he found scientists frequently failed to employ logical reasoning, to apprehend the obvious and correct solution to a problem, or to eliminate subjectivity and metaphysics.<sup>46</sup> As to the second, they were often unruly social agents who would resist acknowledging another's achievements, jealously guard their own findings, and impulsively persevere in a research tactic without any corroboration. The latter, basically "social," problems became a case study from which Hull began to construct a general theory of social behavior. The theory would explain the counterintuitive occurrence of such social behaviors as cooperation, altruism, eminence, prestige, and imitation. These so-called social acts needed no special theoretical constructs, for Hull believed that they could be explained parsimoniously by reference to individual behavioral acts—specifically, to reinforcements and drive reduction in the individual organism. Just as Hull believed that the failure to achieve coordinated behavior was due to conditions of the environment, so too he thought that the insolvency of coordinated and unified scientific research was due to defects of the institution. The question of what enabled successful joint scientific undertakings was identical with the question of why animals share resources, the latter being a problem that he loosely connected with the existence of social dominance. Thus his speculations on the problems of the IHR frequently evolved into discourse on a fundamental theory of social behavior and then into specific designs for experiments on the social behavior of birds, rats, the feeble-minded, or children.<sup>47</sup>

Hull conjectured that a seminar format would enable the participating scientists to provide checks on each others' reasoning processes and force a collective or "semi-communitistic" project to emerge; hence, it would reduce irrational and antisocial tendencies in the group. He remained concerned that a seminar might still digress into "public controversy" or fail to prevent the hoarding of research ideas.<sup>48</sup> Yet his ideal research model, though for the present unimplemented, would eliminate these contaminants and guarantee the rational and logical production of knowledge. Scientific research, he thought, could proceed most economically if structured like a psychic machine, an automatic mechanism free from subjectivity.

<sup>45</sup> "Seminar Notes, I.H.R.," 1936, Hull Papers.

<sup>46</sup> Clark L. Hull, "Simple Trial-and-Error Learning: A Study in Psychological Theory," *Psychol. Rev.*, 1930, 37:241–256; Hull, "Knowledge and Purpose as Habit Mechanisms," *Psychol. Rev.*, 1930, 37:511–525; Hull, "The Mechanism of the Assembly of Behavior Segments in Novel Combinations Suitable for Problem Solution," *Psychol. Rev.*, 1935, 42:219–245; and Hull, "The Conflicting Psychologies of Learning—a Way Out," *Psychol. Rev.*, 1935, 42:491–516.

<sup>47</sup> Hull suggested that experiments on food sharing among animals, specifically the feeding behavior of mother birds, would uncover a "sharing psychology" and thus provide a model for attaining altruistic and cooperative research: "Idea Book," 1935/36, "Seminar Notes, I.H.R.," 1936, Hull Papers.

<sup>48</sup> "Seminar Notes, I.H.R.," 1936.

Hull offered a behavioral explanation for scientific thinking whereby the purported components of intelligence, insight, and purposiveness actually are explicable in terms of a long trial-and-error process.<sup>49</sup> The process is actually a result of the conditioned reflex, “an automatic trial-and-error mechanism which mediates blindly but beautifully, the adjustment of the organism to a complex environment.” In scientific work these activities become systematized, metaphysical entities are discarded, and empirical tests perfect the mechanism. Once the behavioral explanation of mental states is derived, “it should be a matter of no great difficulty to construct parallel inanimate mechanisms, even from inorganic materials, which will genuinely manifest the qualities of intelligence, insight and purpose, and which in so far, will be truly psychic.”<sup>50</sup> The psychic machine that would produce the higher mental acts of reasoning, intelligence, insight, and foresight had an analogue at the level of scientific labor. The building of unified theory demanded an assembly line of workers from all the sciences. Physics would supply the deductive base of the mechanical system of scientific work.<sup>51</sup> Within psychology, tasks would be divided according to their logical and mathematical and their empirical components. Hull conjectured that the scientific project required the making of hundreds of equations and empirical constants and thousands of theorems and experiments: “This great task can be no more than begun by the present generation of workers. Hope lies, as always, in the oncoming youth, those now in training and those to be trained in the future. Upon them rests the burden of the grinding and often thankless labor involved.” Given the trial-and-error nature of all learning and the magnitudinous undertaking of unified theory, scientific work would generate some error—but nothing compared to the folly produced with the current disorderly practices and individualism among researchers.<sup>52</sup> With the mechanical system, the theoretical “chaos” in the house of psychology would be put in “order” and unified science would be unfolded.<sup>53</sup>

As an incarnation of Hull’s methodological mechanics, the Institute seminar would approximate the ideal of scientific practice and would eventually yield integrated and unified science. The assignment of experimental duties to various members would complete the logical empirical engine. And the topic of psychoanalysis and behaviorism was an especially well-suited fuel. To Hull, Freudian psychology best symbolized unwanted subjectivity, the hazardous pollution of illogic and metaphysics.<sup>54</sup> Comparison of the two theories really meant subsuming psychoanalysis in the theory of stimulus-response conditioning. The project simply required the logical translating and operationalizing of psychoanalytic concepts and the experimental testing of hypotheses derived from psychoanalytic theory. To Hull fell the responsibility of demonstrating the superiority of the stimulus-response system, a task that required both his scientific expertise and

<sup>49</sup> Hull, “Simple Trial-and-Error Learning” (cit. n. 46), p. 252.

<sup>50</sup> Clark L. Hull, “A Functional Interpretation of the Conditioned Reflex,” *Psychol. Rev.*, 1929, 36:498–511, on p. 498 and Hull, “Simple Trial-and-Error Learning” (cit. n. 46), p. 256.

<sup>51</sup> Clark L. Hull, “Mind, Mechanism, and Adaptive Behavior,” *Psychol. Rev.*, 1937, 44:1–32, on p. 29.

<sup>52</sup> Clark L. Hull, *Principles of Behavior* (New York: Appleton-Century, 1943), p. 401.

<sup>53</sup> Hull, “Conflicting Psychologies of Learning” (cit. n. 46), p. 513. Elsewhere Hull portrayed psychology as being caught in the self-reproducing sterility of the Middle Ages: see Hull, “Mind, Mechanism, and Adaptive Behavior” (cit. n. 51), p. 32.

<sup>54</sup> Hull’s critical unpacking of Freudian theory appears in his notebook entries as early as 1915: “Idea Books,” Hull Papers.

his management skills in the seminar.<sup>55</sup> That a number of Institute members had psychoanalytic training and that two additional psychoanalytically trained faculty members, Erik Erikson and Earl Zinn, were hired in 1935 and 1936 merely heightened Hull's self-attributed responsibilities and broadened his opportunities to utilize others' training for his purposes.

Hull's program flourished, and his leadership was respected. Although Hull himself preached democracy and believed that the führer-like figure needed at the Institute contradicted American ideals, he solemnly acknowledged his crucial role in inducing the members to work toward cooperation, integration, and a unified science of behavior.<sup>56</sup> Others willingly praised Hull's influence, and May eventually designated him the "planning and supervising architect, while his collaborators have checked the plans and supplied much of the technical skill necessary for their execution."<sup>57</sup> Nevertheless, Hull was not openly regarded as a genius to be venerated or a corporate head to be heeded but as the conveyor of a scientific methodology that approximated the ideal of physics and the perfect research machine. The *methodology* of that working community demanded discipline, control, and order. It eradicated subjectivity, individual idiosyncrasies, and social deviance.

#### TOWARD UNIFIED SCIENCE

The success of Hull's planning depended upon the compliance of other Institute members. Since many of the components had also been proposed by others, and some had been implemented independently of his suggestions, its overall acceptance entailed no radical transformation. The orderly structure and simple rhetoric of scientific processes were familiar and alluring. Equally appealing was the supposedly unprejudiced receptivity to the most problematical of theories, psychoanalysis, and to nonpsychological concepts such as culture, history, and social structure. Hull's was a *rigorous* systematization; it appeared to threaten no particular research project while it unequivocally defined real and orderly relations between them all. The IHR participants had moved toward centralization of organization (through the directorship), regulation of social arrangements (through seminars and division of research labors), and structuring of research (through a psychological metatheory of the individual and environment and a mechanized methodology). These structural changes introduced a shared language to replace the idiosyncratic dialogues of individual researchers. The revised language of community did not so much represent an abrupt shift to psychological, formal, and logical theorizing as codify interests that had been in ascendance for some time. The new framework—linguistic and social—corrected a lag in researchers' self-conceptions. Although by the early twentieth century human scientists were acknowledging that human action is multicausal, interdependent, amoral, and irrational, they had not yet accommodated their self-conceptions to this modern outlook. Nor had they accepted the uncertainties of

<sup>55</sup> See "Seminar Notes, I.H.R.," 1936.

<sup>56</sup> "Clark L. Hull" (cit. n. 44), p. 156.

<sup>57</sup> Mark May, foreword to Clark L. Hull, Carl I. Hovland, Robert T. Ross, Marshall Hall, Donald T. Perkins, and Frederic B. Fitch, *Mathematico-Deductive Theory of Rote Learning: A Study in Scientific Methodology* (New Haven, Conn.: Yale Univ. Press, 1940), p. ix.



**Figure 3.** Building housing the Institute of Human Relations (left) and the Sterling Hall of Medicine (right). Courtesy of Sterling Library.

pragmatism. For those actively working in the IHR, the move to corporate organization and an orderly methodology represented that accommodation.

In a short time the seminars produced cooperative and integrative works. Hull's Wednesday night seminar immediately attracted devotees, and its progress was meticulously recorded in mimeographed notes. Within a year other seminars were established, one on concepts central to an integrated social science and others on special topics such as learning, culture, and psychoanalytic theory.<sup>58</sup> The success of the seminars convinced many members that integrated research could not come from studies of social problems or from data banks filled with social facts but only from *theoretical* and *methodological* convergence. Psychoanalysis provided the community not just with fertile ideas but also with identity: this precarious "clinical" theory offered something of a collective projective test where members could concur at once on the truths of human nature and on the inadequacy of clinical or subjective methodologies. The seminar revolved around the question, not "Psychoanalysis or no psychoanalysis?" but rather "How much psychoanalysis?" The solutions entailed making Freud's work scientifically verifiable and logically coherent. The clinician and the experimentalist, the behaviorist and the psychoanalyst, the analyzed and the unanalyzed, could often reach agreement with a shared methodology and division of work. In addition, physical scientists were occasionally invited to the seminar to demonstrate how methodological orderliness leads to unity. They were perceived

<sup>58</sup> On Hull's seminar, see Clark L. Hull and O. H. Mowrer, "Hull's Psychological Seminars, 1936–38"; and Clark L. Hull, "Psychology Seminar Memoranda, 1939–1940," IHR Archives. On the general seminar, see Monday-Night Group Notes, typescript, IHR Archives. Other seminars are described in May, "Retrospective View of the IHR" (cit. n. 34), pp. 166–167.

to have the correct mental mechanisms (primarily deductive and inductive logic), the "courage," and the aesthetic tastes required to complete the tremendous tasks at hand. To acquaint themselves with these mental abilities, seminar participants were requested to read Newton's *Principia*.<sup>59</sup>

Although the changes were primarily methodological and less in underlying assumptions about human behavior, the consolidation of research engendered by the Institute's new organization had almost immediate fruits. The publications bore the mark of what came to be recognized as the "logical empirical" methodology. Earlier works lacked the novel terminology but not the guiding assumptions.<sup>60</sup> The first large-scale project was a study of aggression conducted by eight members and published in 1939. The researchers employed Hull's model of identifying logical and empirical components of the research problem along with Freud's account of frustration; they derived the hypothesis that "aggression is always a consequence of frustration." Using Freud's claim that aggression follows when an individual is frustrated in his or her striving for some pleasurable outcome, the frustration-aggression hypothesis, perhaps more lucidly than any other Institute project, contrasted the irrational, impulsive, and amoral stuff of human nature with the rational, orderly, and rigorous procedures of a hypothetico-deductive method. The very act of cooperative research, though admittedly frustrating, indicated the potential of the methodology to transcend (or suppress) irrational tendencies.<sup>61</sup> Soon afterward another collectively authored volume on rote learning appeared, along with a plethora of experimental studies grounded in the new integrative social science approach.<sup>62</sup>

Accompanying the rhetoric championing mathematical and deductive methods—on the supremacy of a specific scientific reasoning—was the presupposition that all human behavior consists of individual adaptations to the environment. Governed by the inner workings of habits and drives, these adjustments were held to be blind yet to serve the individual's pleasure seeking; environmental adaptations ensure the individual's survival in an indifferent biological and social world. Whether "inner" or overt, the behavioral habits of the individual organism were held to be the atoms amenable to study through an integrated science; neither physiological processes, social structure, nor unobservable psychical entities were essential to explaining human behavior. What was absolutely necessary was a methodology free of primitive social practices (metaphysics, unprincipled or profligate behavior) and faulty inner habits (emotionalism, partiality, irrationality), along with a community of workers willing to control their own natural tendencies to behave unscientifically.

The evident moves toward unified theory occurred even as Rockefeller officials decided to curtail support of the Institute, awarding only a terminal grant of \$700,000 for the period 1939–1949. Although Foundation officials were optimistic

<sup>59</sup> Hull and Mowrer, "Hull's Psychological Seminars," pp. 35–37, 57–59, 64.

<sup>60</sup> Several works by Institute members contained variations on Hull's system; see, e.g., John Dollard, *Caste and Class in a Southern Town* (New Haven, Conn.: Yale Univ. Press, 1937); and Edward S. Robinson, *Law and the Lawyers* (New York: Macmillan, 1935). For a comparative account of behaviorism and the philosophy of science see Laurence D. Smith, "Behaviorism and Logical Positivism: A Revised Account of the Alliances" (Ph.D. diss., Univ. New Hampshire, 1983).

<sup>61</sup> John Dollard, Leonard W. Doob, Neal E. Miller, O. H. Mowrer, Robert R. Sears, Clellan S. Ford, Carl I. Hovland, and Richard T. Sollenberger, *Frustration and Aggression* (New Haven, Conn.: Yale Univ. Press, 1939), pp. 1, vii.

<sup>62</sup> Hull et al., *Mathematico-Deductive Theory of Rote Learning* (cit. n. 57).

about current operations, they were critical of the earlier failures in productivity and organization.<sup>63</sup> The unfavorable decision undoubtedly was also influenced by the Foundation's diminished interest in pure research in the social and psychological sciences.<sup>64</sup> Nevertheless, the terminal grant was sufficient to maintain the Institute, and with the entry of the United States into World War II, the attention of the IHR members was shifted to war-related activities.

In 1950 Mark May reviewed the first twenty years' work and outlined the task that lay ahead for the Institute: continued integration of the biological and social sciences and research on "inner habits" and "inner speech." But the most immediate problem of the IHR was that of financing, and the results were both partial and temporary. A Ford Foundation grant enabled the Institute to hire five post-doctoral researchers, representing five disciplines, who were to develop an integrative theory. Like their predecessors, these researchers convened in seminars, published a collectively authored text, and ultimately reproduced the achievements of Hull's system.<sup>65</sup>

### CONCLUSION

In 1939 *Time* published a brief account of the Institute that troubled many of its personnel. Entitled, "For Freud, For Society, For Yale," the article intimated that the similarities between the pretentious and phony temple of science in Sinclair Lewis's *Arrowsmith* and the IHR went further than the fact that they were both located on a Cedar Street.<sup>66</sup> In no small manner the Institute did attempt to serve Freud, society, and the university. Yet it also served a crucial social function for human scientists who were adopting assumptions about an irrational and impulsive human nature, an uncertain reality, a disorderly society in need of control, and a segmented intellectual community requiring unification. Such intellectual interests had become commonplace in the early twentieth century, but human scientists had yet to confront some unanticipated consequences of this revised intellectual outlook. When viewed reflexively (when the ideas were applied to the activities of human scientists), that outlook challenged the human scientists' vantage point as rational and objective knowers. However, residual commitments provided an escape, a return to certainty—and hence an ultimate failure to consider the newer ideas seriously. The IHR researchers, like many

<sup>63</sup> Alan Gregg interview with Edmund E. Day, 21 Apr. 1937; Alan Gregg diary, 18 Feb. 1938; "Memorandum on Yale Institute of Human Relations—March, 1938," Rockefeller Foundation Archives. See also Raymond B. Fosdick, *The Story of the Rockefeller Foundation* (New York: Harper & Row, 1952), p. 196.

<sup>64</sup> It appears that during the 1930s the human sciences came to be considered more as applied science, in comparison to the pure science of molecular biology, and that interest in social sciences declined at the end of the 1930s. See Stanley Coben, "Foundation Officials and Fellowships: Innovation in the Patronage of Science," *Minerva*, 1976, 14:225–240; Elizabeth Lomax, "The Laura Spelman Rockefeller Memorial: Some of Its Contributions to Early Research in Child Development," *J. Hist. Behav. Sci.*, 1977, 13:283–294; Robert Kohler, "A Policy for the Advancement of Science: The Rockefeller Foundation, 1924–29," *Minerva*, 1978, 16:480–515; and Kohler, "The Management of Science: The Experience of Warren Weaver and the Rockefeller Foundation Programme in Molecular Biology," *Minerva*, 1976, 14:279–306.

<sup>65</sup> Mark May, *Toward a Science of Human Behavior: A Survey of the Work of the Institute of Human Relations through Two Decades, 1929–1949* (New Haven, Conn.: Yale Univ. Press, 1950), pp. 28–34; and Frank A. Logan, David L. Olmstead, Burton S. Rosner, Richard D. Schwartz, and Carl M. Stevens, *Behavior Theory and Social Science* (New Haven, Conn.: Yale Univ. Press, 1955).

<sup>66</sup> "For Freud, for Society, for Yale," *Time*, 6 Mar. 1939, pp. 41–42.

others, relied on mundane knowledge about social organization. They also returned to tacit notions that the human sciences must ultimately have an anchoring point or some prime unit in reality, that this point was individual behavior, and that they must provide certainty—they must, in fact, be “science” as it was ordinarily construed. The Darwinian revolution, theories of the unconscious, pragmatism, and critiques of positivism and idealism had enlightened intellectuals about human nature but had also inadvertently endangered human scientists’ authority.

The IHR became just one project to locate such foundations and thus to substantiate a model of human nature and science. The product in many ways resembled an earlier world view. The project’s designers recognized the necessity of attaining order within the intellectual community: the “rugged individualism” of the nineteenth-century scholar was no longer a tenable ideal, given the newly acknowledged magnitude of the enterprise and of the inadequacies of the human intellect. Certainty amounted to consensus, although the pragmatic basis of this criterion of truth was rarely noticed. The ideals of democracy in science likewise required revision, because total freedom in intellectual pursuits spelled susceptibility to idiosyncratic reasoning and disintegrated scholarship. Research required organization and control. The traditional ideals of individual enterprise and democratic practices were gradually replaced by ideals specifying control and order, by metaphors of corporate life, and by methods abstracted from the “higher” sciences. In a very real sense the IHR was an experiment in science. This experiment in social arrangements contributed to salvaging the human sciences from the risks of self-reflection and from the pluralism of laissez-faire pragmatist philosophies; it offered a more mechanical, realist, and formalist vision of the human world. Among the disciplinary constituents in this transformation, psychology—the science of the individual and of the so-called less obvious dimensions of social life—was the most assiduous pupil and the chief beneficiary. The experiment yielded certainty for the scientists themselves as well as for their knowledge claims, and it did so through a language of social control. In a retrospective account one IHR participant applauded the multiple virtues of control:

Life would be unbearable in a world where one was constantly having to choose. Uncertainty is exhausting and choice demands special psychological strengths and reserves. It is, therefore, a human necessity that the world be, to some extent, predictable. Behavior must flow along at least some of the time in golden quiet. Man needs orderly knowledge, scientific knowledge, a kind of knowledge which permits him to act most of the time without the excruciating necessity of choice. . . . Orderly knowledge is easier to teach and easier to learn, because one item in theory suggests another. A correct theory simplifies human problems and makes individual choice easier.<sup>67</sup>

Like others, John Dollard was disturbed by the mere perception of choice and the unavoidable circumstance (of reflexivity) that human scientists are both subject and object of their science. The formative decade of organizing researchers at the IHR illustrates several consequences of reflexivity in the human sciences. Among the solutions to the inadvertent reflexive thinking was the belief that

<sup>67</sup> John Dollard, “Yale’s Institute of Human Relations: What Was It?” *Ventures Magazine of Yale Graduate School*, 1964, 3:32–40, on p. 32.

controlled and orderly work arrangements would eliminate subjectivity and the ambiguities of choice. The fate of behaviorism and the recurrent crises in the psychological sciences generally intimate that such problems have not dissipated. Yet just as American human scientists have attempted to dismiss the properties of reflexivity, so those who chart the history of these sciences have embraced practically the same guiding assumptions. These historians tend to accept the presupposition that human scientists, with the possible exception of those doing applied work, can stand somewhere outside culture, history, and even themselves. When the “personal” or “social” involvements of the human scientists are acknowledged, it is usually in terms of idiosyncratic personalities, interpersonal frictions, or simple political biases. Human scientists are rarely perceived as both producers and consumers of, directors of and participants in, cultural knowledge. Perhaps this approach is continued because most histories of the human sciences are prepared by individuals trained in those sciences who hence retain a particular scientific ethos. Whatever the reasons, reconsideration of this presupposition might well introduce substantial changes into historical studies of the human sciences.